

## American Neurological Association.

### THIRD ANNUAL SESSION.

**FIRST DAY'S PROCEEDINGS:** Reports of officers and of Council.—Inaugural address.—Dr. Hammond's case of moral depravity.—Lead poisoning in frogs by Dr. Mason, and discussion.—Report of Dr. Jewell on the structure and functions of the ganglia on the posterior roots of spinal nerves and discussion on the same.—Dr. Schmidt on the structure of the sympathetic ganglionic bodies.—Dr. Mason, structure of spinal cord of American bull-frog.—Dr. Seguin, sections of the spinal cord of the green sea turtle.

**SECOND DAY'S PROCEEDINGS:** Election of members.—Dr. Emerson, syphilitic sciatica.—Dr. Eugene Dupuy, hereditary epilepsy and hereditary deformities, and discussion on the same.—Dr. Shaw, exhibition of a case of locomotor ataxia in a child.—Dr. Hammond, odor of the human body, as developed by certain affections of the nervous system.—Beard, endemic tetanus.—Dr. Rockwell, intermittent hemiplegia, with discussion on same.—Dr. Gibney, spinal irritation, and discussion.—Dr. Seguin, post-hemiplegic chorea.

**THIRD DAY'S PROCEEDINGS:** Election of officers.—Dr. Shaw, brain Tumors, with discussion.—Dr. Dupuy, vaso-motor centres, and discussion of same.—Dr. Seguin, cerebral localizations, discussion.—Dr. Dupuy, hereditary transmission of peculiarities.—Dr. Beard, influence of the mind in disease.

### WEDNESDAY, JUNE 6TH.—AFTERNOON MEETING.

The American Neurological Association convened at the College of the Physicians and Surgeons, in New York City, June 6, 1877, and was called to order at 2:30 P. M., by the President, Dr. J. S. Jewell, of Chicago.

Present: Drs. Jewell, Miles, Hammond, Mason, Shaw, Emerson, Beard, Loring, Cross, McBride, Kinnicutt, Seguin.

As the minutes of the preceding session had been printed and distributed among the members of the Association, on motion, their reading was dispensed with.

#### REPORTS.

DR. E. C. SEGUIN, of New York, Secretary of the Council, made the Annual Report, which was adopted.

DR. J. J. MASON, of New York, Corresponding Secretary of the Association, made his report, which was accepted.

DR. E. C. SEGUIN, Recording Secretary and Treasurer of the Association, made his report, which was accepted.

At the recommendation of the Council, the resignations of Dr. J. W. S. Arnold, of New York, and Dr. F. D. Lente, of Florida, were accepted.

It was voted that the Recording Secretary present Dr. William Detmold with the thanks of the Association for the use of the room.

Dr. William A. Hammond and Dr. N. B. Emerson submitted new by-laws.

THE COMMITTEE ON NOMINATIONS,

as appointed by President Jewell, was as follows: Drs. Miles, of Baltimore; Shaw, of Brooklyn; Kinnicutt, Cross, and Emerson, of New York.

The business of the first meeting having been completed, the

INAUGURAL ADDRESS

of the session was delivered by the President, DR. J. S. JEWELL.

The following is an abstract of the address:

"GENTLEMEN OF THE AMERICAN NEUROLOGICAL ASSOCIATION:—

At this our third annual meeting, it has seemed to me appropriate that I should deliver at its opening a short address, as a means of enabling me to discuss certain matters which can hardly find suitable expression at any other time, or in any other way. The nervous system in its totality, which is professedly the object of our consideration, has striking peculiarities as a field for scientific and practical study. From whatever stand-point it may be considered, more unexplored territory lies within the confines of the spinal cord, medulla, and brain than in any other part of the organism. There is not a part of the body, it is probable, into which it does not penetrate, and hence with which it does not establish relations. This is, on the one hand, for the purpose, so to speak, of being always informed as to the varying conditions of all parts of the organism, and on the other hand, for the purpose of exerting an influence upon the same. While it is, in the strictest sense a special field, it is so happily situated as to give those who enter and cultivate it in a rational spirit no excuse for that narrowness of thought and sympathy so often and so unhappily found among those who cultivate specialties.

"Such, in brief, is the field of our study. In its cultivation for the future there are several things I should be glad to see realized.

"The first is, that in this country, henceforth, more attention and encouragement may be given to a thoughtful study of the healthy anatomy and physiology of the nervous system. No doubt there are many now among us who are endeavoring to keep pace with progress in these fundamental de-

partments of neurological science, and some few are endeavoring to confirm or enlarge the boundaries of actually existing knowledge; but it cannot be denied that thus far very little, comparatively, has been done and made public, in our own country, towards advancing a knowledge of the normal anatomy and physiology of the nervous system. The reasons for its neglect thus far, whether good or bad, have now in a great measure passed away. The time has now come—and with it the opportunities—when we should undertake to make some solid contribution in this department of our work. To excite and encourage, and beyond this to prosecute such researches, should be one chief object of the existence of such a society as ours.

“Then again, we need not less experiment, but more care as to methods and results. There can be no question in my mind that there is, relatively speaking, too little close, accurate thought as compared with the mere observation of facts. The mere discovery of a new fact by sense observation does not insure that the discoverer will ascertain its significance. What I mean to declare is, that mere sense observation has outstripped, and does this very hour outstrip, critical, careful thought. What we need is, not mere observers nor mere thinkers, but more men, who like Bacon, Harvey, Bichat, and others, can not only observe, but, like them, think. I would be glad to see this society in its work avoid with a set purpose an ill-balanced course in this respect.

“With due respect to those who have labored in less favored times, I wish to see less reliance placed on the records of pathological cases, as they exist in the literature of the past. With a better knowledge of the anatomy and physiology of the body, with a broader range and basis of established facts than ever before, and with greatly improved methods for research, we are able happily to lay a surer foundation, let us hope, for trustworthy deductions.

“I wish now to lay before you a few practical suggestions, as to the organization and working of the Society.

“I am now, as I have been from the first, of the opinion that the society is about as large as it ought to be until it has lived a little longer and done more good work. I do not say no new members should be admitted, but for a time let them be

few, and admitted with caution. Then, again, the society should continue to hold its next few meetings in the East. The bulk of its members must for a long time be here, and also the means for rendering them attractive. It should meet either in this city, Boston, or Philadelphia, or what may be thought better, in some adjacent place of resort. Then, again, the time of meeting of the Society should be so changed as not to conflict with other important meetings, which many of our members might feel like attending. I would suggest that the meeting be postponed till the second or last week in June, or until September. Then in regard to the publication of our papers and discussions. The only things to be done are either to publish an inexpensive account of the proceedings and abstracts of discussions and papers, or to publish a volume of transactions. Of these two plans, the latter is every way preferable. A subscription of from fifty to one hundred dollars from each member would completely insure the appearance of the volume. To render the work of the Society more effectual, I would recommend a diminution in the number of officers, who should, as now, be aided by a council in discussing purely business questions, which should rarely, if ever, occupy the attention of the Society as a whole; and that the Secretary be charged with the not very onerous duty of editing the materials, employing such aid as he may need. I would also suggest the propriety of the appointment of committees to report at subsequent meetings on definite subjects.

“Such are a few of the suggestions which occur to me, and are offered because of their importance. I had intended, at first, to have made a survey of the general field of neurology and to have indicated with some fullness and particularity the direction and tendency of research, and to have mentioned many of the yet open questions which appear to be within the field of neurological medicine, but it has seemed to me the suggestions which I have made would be of more importance to the Association.

“I will conclude, therefore, gentlemen, in the expression of something more than a hope, that we may, at this meeting, make a new departure, and though our number is small, that we may remember that the utility and renown of this Society

will not depend on its numbers, but on the character of its labors."

Next followed the exhibition of an interesting case, by DR. WILLIAM A. HAMMOND, of New York.

The patient, a boy of 18, the Doctor said, might be considered as a case of total moral depravity. He would lie and steal to any extent, without any apparent reason; would steal clothes and other articles, sell them for a trifle, then give the money away. The boy had been placed in several houses of correction, but they could do nothing with him. In reply to the question, "What makes you do these things?" he said, "I can't help it." He did not seem to be lacking in intelligence. His mother had for a long time noticed blood upon his pillow. Dr. Hammond considered the case one of epilepsy. Weight of child at birth one pound. His father is an exceedingly neurotic individual.

#### REMARKS ON DR. HAMMOND'S CASE.

DR. SEGUIN. Do you know anything of his family history?

DR. HAMMOND. His father is one of the most neurotic subjects I ever saw. He has used tobacco to excess,—smoked and chewed, but now he does not smoke. While the mother was pregnant with this boy, she had a great shock, mentally.

#### LEAD POISONING IN FROGS.

DR. J. J. MASON, of New York, read a paper on this subject which suggested a new field for experiment.

Notwithstanding the important services which experiments upon this animal have rendered, in discovering the mode of action of other toxic substances, not even an allusion to the action of lead upon the system of the frog, can be found in toxicological literature.

The subject was divided into

1. Acute poisoning; 2. Chronic poisoning.

In the former, the poison (acetate of lead) was introduced under the skin; in the latter, by placing the animal in a solution of the same salt.

The acute form of poisoning is characterized by paralysis of the heart with preserved integrity of the motor nerves and muscles; while, in the chronic form, paralysis of the muscles of volition, with their nerves, invariably results, leaving the heart intact.

The muscles always show Erb's *Entartungsreaction* to electricity, a characteristic feature of lead palsy in man. A ready method is here found of inducing this condition of the muscles at will, and its value in the study of pathogeny of lead paralysis suggested to the consideration of the Association.

In view of the little we know on this subject, and of the difficulty of obtaining autopsies in man and failures to induce the same condition in warm-blooded animals, may not much be added to our knowledge by further research on lead poisoning in the frog?

The paper being open for discussion.

REMARKS UPON DR. MASON'S PAPER.

DR. HAMMOND. I would like to ask if there was any evidence of lead in the nervous system?

DR. MASON. I could not say as I have made no such examination.

DR. CROSS. In the human subject lead has been actually found.

DR. JEWELL. In which form of poisoning, acute or chronic, did the paralysis occur?

DR. MASON. In the chronic.

DR. JEWELL. In the acute form without the muscles having lost their excitability, was there paralysis of the heart?

DR. MASON. Yes sir. I have never noticed paralysis of the heart in the chronic form.

DR. JEWELL. You spoke of opening the heart, and that it began to act again, was its action spontaneous?

DR. MASON. Yes. The rapidity of the pulsations was about 24 per minute.

DR. JEWELL. How does that compare with the action of the heart in the healthy animal, when it is extracted from the body when there is no lead poisoning?

DR. MASON. I have seen the heart act, out of the body, three or four hours after death had apparently taken place.

DR. JEWELL. In the case of the acute form of poisoning, was the heart removed from the body? DR. MASON. It was *in situ*. DR. JEWELL. You found no change in the spinal cord, and applied no chemical tests? DR. MASON. No, sir, but I wish to do it.

DR. JEWELL. I have known of a number of cases of paralysis in type setters. About a year or more ago, I had one case of this kind, and he has brought me word of seven or eight more cases of the same kind. Every one of them had paralysis, or paresis, and especially those whose hands were most commonly smeared with the material from the type. In one or two instances it seemed to me that nothing less than a deposit of the poison on the hands and fingers, the nerves could have explained the phenomena present. I have been in doubt in regard to these cases, and hope Dr. Mason will follow the matter up, and especially bring in micro-chemistry to aid him, so as to ascertain definitely whether the lesion is peripheral or central.

DR. HAMMOND TO DR. JEWELL. You speak of your cases as if they were lead poisoning, were they not cases of antimonial poisoning? I am inclined to think it was antimony instead of lead that caused the trouble. In regard to the central lesion, in lead poisoning, I think there is no doubt about that. Against the local absorption theory, I have had cases of lead poisoning from the use of hair dye, where the paralysis was not in the muscles of the face, but in the wrists, and in the case of painters, in the legs.

DR. F. T. MILES. There are some cases which are very hard to explain by considering the trouble central, as for instance, paralysis in the lower extremities.

DR. JEWELL. I have no doubt in my own mind, that the trouble is central, but one or two of the cases which I had, it was a little difficult to explain the phenomena present, except by local absorption. It is true that in the cases I have cited, the poisoning was more probably due to antimony than to lead; but the phenomena were apparently local, and it appeared to me the trouble must be in the nerve trunk itself.

DR. SEGUIN. I had the opportunity of making one observation bearing upon this point. The patient was recovering from lead paralysis at the time he was carried off by a diarrhea. I made an examination of the muscles, which showed the usual degenerative changes. A section through the cervical enlargement showed slight granular change in the ganglionic cells of the anterior horns. At that time, 1874, I took

strong ground in favor of a central affection, and am still disposed to this view, though not so strongly as before. As regards Dr. Jewell's cases, I think they were of antimonial origin. A remarkable fact in regard to lead poisoning is that it does not attack the interossei.

DR. HAMMOND. In my experience printers are very rarely affected.

DR. SEGUIN. I have only seen two cases of lead poisoning in printers. DR. HAMMOND. It is quite rare.

DR. EMERSON. I had a case in which the muscles wasted were those of the thenar and hypothenar eminences.

DR. CROSS. I have given some little thought to this subject, and it has struck me that it was not possible to produce paralysis by mere contact with the hand, from the fact that you can have lead paralysis where there has been no contact, except by means of the respiratory organs, as in the case of smelters. I have seen cases where the muscles in use were paralyzed, and yet the lead had not come in contact with the outer skin, which seems to me strongly against the argument that it is by contact. I believe the affected region is central and high up. Another fact which favors the view of a deposit in the tissues in the case of lead poisoning, is that under the iodide of potassium treatment the symptoms rapidly disappear.

DR. MASON. I should have seen how long a paralyzed frog would live. DR. SEGUIN.—Was there wasting of the muscles? DR. MASON.—No sir. DR. HAMILTON remarked that a few years ago, he had made some investigations in behalf of the board of health, bearing upon the subject. He had seen many cases of paralysis in printers, and had found lead paralysis exceedingly rare among them. Dr. Hamilton cited the case of a girl where the paralysis was, he thought, due to the constant use of the muscles. She was employed in dressing type.

DR. SEGUIN, in reference to the remark which Dr. Cross made in regard to the respiratory tract, I would say that in paint shops, where the assistant painters are required to burn off old paint, previous to repainting, they are liable to lead poisoning from inhalation of the fumes and smoke. Dr. Seguin cited one case which had come under his notice. In lead man-



ufactories the poison was undoubtedly taken into the system by inhalation. Attention was called to the fact that workmen were inveigled into these manufactories without being warned of the dangers.

DR. J. C. SHAW.—I have seen several cases such as Dr. Seguin has called attention to, in which the paralysis has occurred within a week after entering a lead manufacturing establishment.

DR. CROSS.—I would say in regard to the respiratory tract as a means of conveying lead poison into the system, that those who work in European lead mines, are directed to drink diluted sulphuric acid, and by this means the proportion of cases thus originating has been reduced. I would suggest to Dr. Mason, if he continues his investigations, to have the muscles, nerves and spinal cord examined microscopically and chemically, for by so doing he might be able to throw some light upon the subject.

D. MASON.—I have examined the nerves, and they appear to be healthy. As stated in the paper, I have not been able to find any change. I have at present, at least, a half dozen spinal cords undergoing the process of hardening for future microscopical examination.

THE PRESIDENT, DR. J. S. JEWELL, then made a verbal report to the Association of the progress he had made in the examination into the

STRUCTURE AND FUNCTIONS OF THE GANGLIA ON THE POSTERIOR ROOTS  
OF THE SPINAL, AND ALSO OF THE CORRESPONDING PART OF THE  
CRANIAL NERVES.

DR. JEWELL'S remarks were a continuation of a paper upon the same subject, read before the Association at the last annual session.

He stated that his researches were far from being complete, but to him were suggestive. An important question was, as to what became of the fibres which appear to rise from the nerve-cells found in the ganglia in question. Do they pass toward the cord or toward the periphery of the body? The Doctor's first opinion was the same as that held by many others, since the time of the early observations of Koelliker—that they pass toward the periphery—but he has since aban-

doned that opinion. His opinion now is, that they do not pass either way, but that they join the axis-cylinders of the fibres of the sensory root, at the so-called, "constriction of Ranvier," as these fibres pass through the same ganglion. This view he was first led to entertain by seeing preparations of these ganglia made by Dr. Amidon, of New York, and after reading the account of these bodies, given by M. Ranvier, who describes what he calls the "*terminaison en T*." This mode of termination of nerve-fibres in other nerve-fibres, Dr. Jewell has since ascertained, had been described by R. Wagner, of Goettingen.

Dr. Jewell has abandoned his opinion that the nerve cells of the ganglia give off two processes, which connect with either two nerve-fibres, or one other cell and a fibre, or with two cells. He now believes that they are connected with one fibre, and that fibre connects, as already described, with the axis-cylinder of a sensory nerve-fibre, as it passes through the ganglion. But for what purpose does this connection exist? This question, Dr. Jewell thinks, is fully answered by making sections, in the living animal, of the posterior or sensory root, at one time between the cord and ganglion, and at another on the peripheral side of the ganglion. In either case Wallerian degeneration sets in, but in a curious manner. In the case of section on the central side of the ganglia, the degeneration is toward the cord, not toward the ganglion, while in the case of the section on the peripheral side of the ganglion, the degeneration of the nerve-fibres takes place toward the periphery, and not toward the ganglia. These observations show conclusively that the ganglia exert a conservative influence over the fibres of the sensory nerves throughout their whole length, from the periphery to their implantation in the spinal cord. Here, then, we have a highly probable determination as to the function of the nerve-cells in the ganglia on the posterior roots of the spinal nerves. They exert an influence on the nutrition of the fibres of the sensory root. Their function is trophic. They are part of a *trophic nervous system*.

Another question arose as to whether these same ganglionic bodies do not exert an influence on the non-nervous tissues of

the body, through the sensory nerves with which the spinal ganglia, on the one hand, and the ultimate anatomical elements of the tissues, on the other, stand in such intimate relations. Dr. Jewell announced that it was his conviction that such is the case. The nutrition of most parts of the body is, to a certain extent, and in a certain manner, under the control of the mechanisms found in the ganglia. Dr. Jewell did not want it understood that it was his opinion, that the nutrition of the body *depends* on either the spinal ganglia or any other part of the nervous system, but that it is to a certain extent under its control. Disease of the ganglia, and also of the spinal cord, may lead to such changes in nutrition as to produce the so-called idiopathic inflammations we so often witness in the skin and other parts of the body, and indeed any cases involving nutritive change, side by side with changes in blood-supply, which cannot be fairly connected with a local injury, mechanical, chemical, or otherwise. As regards the vascular changes which follow in the wake of irritative tissue change, of presumed neurotic origin, they are to be explained on quite different grounds, since they occur through the agency of a different class of nervous mechanisms. It was Dr. Jewell's opinion that the spinal cord contains in connection with the medulla, and possibly the brain, a central mechanism, which may be properly called trophic, and that the spinal ganglia probably bear the same relation to it that the ganglia on the fundamental chain of the sympathetic, so-called, do to the central vaso-motor mechanisms of the cord and medulla. Dr. Jewell stated that it would not be possible for him at present to go at length into the reasons for his beliefs, though he would certainly do so before long. He had simply desired to report progress.

Dr. Jewell expressed a desire to have the subject of his paper discussed.

#### REMARKS UPON DR. JEWELL'S PAPER.

DR. HAMILTON inquired the way in which atrophy occurred.

DR. JEWELL replied, using the blackboard to illustrate his remarks, for instance as in progressive facial atrophy, a certain nerve-fibre, I will suppose, terminates centrally in the cord by two or more filaments; I suppose this fibre to be connected,

directly or indirectly, with several distinct groups of cells. One of these groups is supposed to have a trophic function, another has a sensory function, another group some other function; now it is possible to have this trophic group the seat of a disease which shall exert an influence along the fibre, with which it stands connected, while its sensory function remains intact, because its sensory group of cells remains normal. My belief is that the sensory nerve-fibre conducts all kinds of sense impressions, after the same fashion as the wire of the telephone which is used to convey several messages in the same direction or in opposite directions, at the same time. This supposed trophic tract in the cord, I expect to see sometime demonstrated. Only in the way I have indicated can I explain certain local inflammations or local atrophies which take place without apparent cause, and seem to follow in the wake of nervous disease. DR. HAMILTON.—I simply spoke of this because of its interest.

DR. MASON.—You believe that to be a motor fibre? (Pointing to Dr. Jewell's diagram.) DR. JEWELL.—No sir, for sensory fibres go to the muscles. DR. SEGUIN.—There is one fact which makes it unnecessary for Dr. Jewell to suppose that sensory fibres go the muscles. I have one case of progressive facial atrophy in which I cannot admit that there is any muscular atrophy. The changes are chiefly in the skin and bones. DR. HAMMOND.—There is a difference between simple atrophy and degeneration. DR. CROSS.—You cannot tell whether there be anatomical atrophy, by means of the electrical current. DR. SEGUIN.—The reactions in my case were so absolutely natural, that I could not admit the existence of any pathological atrophy. With respect to Dr. Jewell's theory, Dr. Mitchell has advanced the view that in cases of alteration of nutrition, the lesion is an irritative one, and this view has also been held by Dr. Brown-Séquard and others. In Dr. Brown-Séquard's laboratory, I have repeatedly seen Guinea-pigs whose sciatic nerves had been cut, remain indefinitely without ulcerations of the feet, but if these animals were neglected, and the feet allowed to remain in filth and urine, frightful ulcerations ensued.

DR. HAMILTON.—Some of the most interesting changes occurring in joints, are those found in locomotor ataxia.

DR. SEGUIN.—I believe that M. Charcot has tried to show, and in one case has demonstrated that the anterior horns were the seat of lesion when arthropathy had been present. In those cases the alteration of nutrition is not connected with the lesion of the ganglia.

DR. HAMILTON.—The cases which I referred to were those in which there is no paralysis. DR. JEWELL.—In regard to the character of the disorders in the nervous system that give rise to inflammations, I have no doubt that they must be irritative.

\* \* \* I have said that there is probably a central spinal apparatus which is brought into play, for the regulation of the production of heat, which is summed up possibly in the medulla oblongata; that this apparatus probably exerts an influence on the nutrition of the body. The central apparatus, if it exists, I have no doubt is in the spinal cord. Some idea of this mode of action may be obtained by analogy. You can excite the secretions of certain glands by irritating their nerves, even after the nerves going to them have been cut. Now the changes going on in a gland in the act of secretion, appear to me to be of the nature of nutritive acts, but they are under the control of the nervous system. In my own mind there is a strong conviction that there is a portion of the nervous system set apart for the control of the nutrition of the body.

DR. SEGUIN. It seems to me that there is one condition that escapes from Dr. Jewell's mind. As I understood him, the supposed irritation at this point in the sketch, causes an influence to go along the nerve trunk, and in that way sets up a change in nutrition. Opposed to such a theory as that is the fact that the nerve trunk in four days after injury, loses its power to conduct irritations, that is after complete section. Unless we suppose regeneration to take place, conduction could not occur.

DR. JEWELL. In reply I would say, that such cases as I refer to, would not be represented by that supposed by Dr. Seguin. I am not referring to divided or mechanically injured nerves so much as to those which arise from the cord, at some point, where the gray matter with which the fibres stand re-

lated, is in a state of *irritative disease*. I suppose an influence to be exerted from the seat of disease, just mentioned, through the fibre, on the nutrition of the parts to which the fibre in question goes, which causes disturbances of the nutrition of the part. *Irritative* disease of a nerve trunk, or of a spinal ganglion, I suppose to be followed often by similar results. It is not necessary in such cases to suppose Wallerian degeneration, or any other form, which destroys the conducting power of a nerve.

DR. MASON. There is one point I have not settled, in regard to your remarks. How does it make the matter any more clear to conceive of an irritation starting in a single cell T, in your diagram, than to think it starts in the lungs, or that it starts in any number of cells, or without any nervous impression? It is just as easy for me to think of an irritative influence starting in one place as in another; it must start somewhere.

DR. JEWELL. I do not see how a local so-called idiopathic inflammation can be excited in a protected part, as it often is, unless by means of some disturbing influence conveyed to it by the way of its nerves. If certain parts of the nervous system are capable of exerting an influence on the nutritive activities of related parts of the body, after the manner of certain other parts of the nervous system, on the muscles or secreting organs of the body, I do not see why it is not easier to imagine local irritations arising in this way in non-nervous parts, than *de novo*. Certainly, a nervous impulse passes along a nerve to a muscle, which makes it contract, or along a nerve which modifies the action of a gland, and it is in some such way, that I suppose, the nervous system to act in modifying the nutrition of a part. Why refuse to believe it possible for the nervous system to exert such an influence? This whole matter of a "heat regulating" function of the nervous system requires it to exert such an influence.

#### FIRST DAY—EVENING SESSION.

The Association was called to order by President Jewell, at 8 P. M.

Present: Drs. Jewell, Miles, Shaw, Emerson, Mason, Kinnicut, Beard, Rockwell, and Seguin.

The first paper was by Dr. H. D. Schmidt, of New Orleans, who was unable to meet the Association in person the present session. The paper was read by the Secretary, Dr. Seguin. The communication was accompanied by a number of thin microscopic sections, of different ganglia of the sympathetic nervous system, and also some sketches of ganglionic bodies.

The object of Dr. Schmidt's communication was not only to offer an opportunity to those persons willing to devote the necessary time to the examination of the specimens, of convincing themselves of the truth of his former statements, but moreover to direct the particular attention of the Association to the structure of these bodies, each of which seems to represent a complete nervous apparatus in itself, and, also, to the importance attached to a true knowledge of the particular mode in which the sympathetic nervous system operates. As the investigation of the structure of these bodies is one of the most difficult in histology, the Doctor considered it necessary that the investigator, in order to recognize this structure, should be able to represent in his mind the exact form in which sections of a complicated body of a certain form, made in different directions, would appear to the eye. Dr. Schmidt called attention to the fact that in examining the ganglionic bodies, lodged between the double contour nerve-fibres of the *plexus gangliformis* of the pneumogastric nerve, the meshes of the net-work forming the capsule, would be found much coarser, a fact which rendered these bodies very suitable for research. Attention was called to the fact that from each sympathetic ganglionic body, two different kinds of processes arise; the larger of these, having the appearance of true axis-cylinders, are most probably transformed into double contour nerve-fibres, while the finer and shorter ones contribute to the formation of the nervous net-work of the capsule, from which the fine fibrillæ, forming eventually the sympathetic nerve-fibres, arise. Thus we have two different kinds of nerve-fibres arising from one and the same ganglionic body. Now the question arises, does this arrangement bear any relation to the rhythmical or peristaltic action of the involuntary muscles? And

further, is one kind of nerve-fibre motor while the other is inhibitory? If so, which are the sensory fibres? Another question to be answered is, whether those sympathetic ganglionic bodies, with the nerve-fibre arising from them, lodged in the plexus gangliiformis of the pneumogastric-nerve, bear any relation to the inhibitory action of this nerve? Dr. Schmidt's view is that difference in structure conditions a difference in function.

## REMARKS ON DR. SCHMIDT'S PAPER.

DR. JEWELL. This finished drawing of Dr. Schmidt's is almost like one by Dr. J. Arnold, of Heidelberg. Dr. Schmidt's labors have been conducted in the South, at a time when he could not have had very much literary aid, and hence he may not have seen Arnold's paper. This peculiar reticulated appearance, Courvoisier and Schwalbe, do not explain, as do Arnold and Dr. Schmidt, that instead of being protoplasmic threads meandering over the capsule of the cell, the reticulated appearance is probably due to the lines of apposition of epithelial bodies which cover the ganglion cell walls.

DR. SEGUIN. It seems to me, Mr. President, that there have been a good many illusions of vision in looking at nerve cells. I have looked at ganglion cells a great deal, and have been unable to recognize many of the figures described by certain writers. I think they have been figured wrongly. I refer to the nucleus and nucleolus threads which are described by some. When I had the privilege of studying in Prof. Max Schultze's laboratory, I asked him in regard to these points, and he said that he had never seen anything of them, and considered them an illusion. He considered the various appearances as due to fibrillation, and he explained to me orally that working with poor instruments had made this mistake. And so, too, about the cells with respect to the connection of the axis cylinder with the nucleus: Some have figured cells with an axis cylinder beginning in the nucleus, all of which Dr. Schultze assured me he had never seen.

DR. MILES. Is there not a breaking up of the axis cylinder into fibrillæ?

DR. SEGUIN. I have looked very faithfully for these, but



have never seen them, even with an immersion 15th of Wales.

**DR. JEWELL.** Dr. Schmidt has some material on this subject. That Dr. Schmidt is an accurate and able observer, I believe no one will doubt. He says he has found rows of granules which seem to be the early axis cylinder, so disposed as to reach the nucleus of the cell. Arndt figures very nearly the same thing, after careful research, and he represents the axis cylinder as breaking up into a sort of spray, extending quite as far as the nucleus, so as to partly obscure it.

**DR. MASON** then gave an informal demonstration of the structure of the spinal cord of the American bull frog, and the medulla oblongata and spinal cord of the alligator, by means of fresh and permanent microscopic specimens. Several microscopes were provided for the purpose.

**DR. SEGUIN** showed a number of transverse sections of the spinal cord of the green sea-turtle, exhibiting remarkably long cell processes proceeding from the anterior horns into the white columns. There were also shown three photographs of the same specimens, made by Dr. J. W. S. Arnold.

At 9:20 p. m. the Association adjourned to attend a reception at the residence of Dr. William A. Hammond, to which he had invited the Association and a number of the profession in New York.

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#### TUESDAY, JUNE 7TH.—AFTERNOON MEETING.

The Association was called to order at 2 o'clock by President **JEWELL**.

Present: Drs. Jewell, Miles, Hammond, Mason, Dupuy, Emerson, Beard, Rockwell, Shaw, Kinnicutt, McBride, Hamilton, Loring, Seguin.

The Secretary read the report of the Council recommending the following gentlemen for election as active members: Dr. V. P. Gibney, and Dr. E. C. Spitzka, both of New York. These gentlemen were unanimously elected.

The first paper was read by Dr. N. B. Emerson, of New York, and entitled

#### SYPHILITIC SCIATICA.

**DR. EMERSON** called attention to the fact that syphilis is very

infrequently recognized as a cause of sciatica. Out of the reports of sixty-three medical men, in answer to Lauder Brunton's questions in regard to sciatica, only twelve spoke of the syphilitic form.

There was sufficient clinical and post-mortem evidence for making the statement that syphilitic neoplasms are capable of producing violent pains and loss of motor function in the sciatic nerve by pressure. Cases were given to substantiate this point. The Doctor considered it established that the syphilitic diathesis is capable of producing neuralgia in the sciatic nerve in some occult manner, without lesion. His reasons are, in brief, as follows:

1. Cachexia is acknowledged to be a frequent cause of neuralgia.
2. Syphilis is a notable cause of cachexia.
3. Syphilitic cachexia and neuralgia are frequently found to coexist, and with relief of cachexia comes relief of neuralgia.

For a diagnosis of syphilitic sciatica, the patient must have been free from sciatica previous to the syphilitic attack; an attack in the late stage of syphilis is more significant than in the early. The prognosis of syphilitic sciatica is the same as that of the syphilis which causes it. The treatment is practically that of syphilis; opiates may be necessary for the relief of pain; ferruginous tonic remedies should be made use of for the relief of the cachexia.

There was no discussion on this paper.

The next paper was read by Dr. Eugene Dupuy, of New York, on

#### HEREDITARY EPILEPSY.

It is well known that Dr. Brown-Séquard has discovered that certain lesions of the spinal cord, or the brain, or the sciatic nerve, in Guinea-pigs, will give rise to an epileptic malady in these animals. In from three to six weeks after the operation, it is found that an alteration in the nutrition takes place in an area of skin, which is limited by a line starting from the outer canthus of the eye, and running to the median line on the upper lip enclosing the nostril, thence backward, enclosing the lower jaw to the anterior portion of the

shoulder to the median dorsal line, to the base of the ear and inner canthus of the eye. The alteration in nutrition takes place on the side corresponding to the injury. It consists in this, that the faculty of feeling pain, heat and cold, disappear by degrees, while tactile sensation appears to be exalted. In a few days it is found that tickling this zone of skin will give rise to twitching which are limited to the muscles of the eye and the eyelids on the same side. Later, the muscles of the mouth and of the face partake; still later the contractions become more general, until this whole side becomes the seat of convulsions; then the convulsions attack the other side also.

When things have come to this point the convulsions precede, by a very short time, a complete loss of consciousness. If the subject of experiment be a white Guinea-pig, it is found that there is paleness of the face, but in all cases there is a little foam at the mouth, and dilatation of the pupils. In some cases the animal utters a cry, probably corresponding to the epileptic cry in the human species. Not only are the convulsions identical with those in epileptic man, but there is also loss of consciousness, a state of torpor, stupidity, and even in some cases something like insanity.

It happens that such animals recover spontaneously, and in so doing, all of the phenomena described above occur in a reverse order, and the zone of skin regains its lost functions. When the epilepsy is due to the destruction of the sciatic nerve, the foot of that side loses the two outer toes, so that the animal has only one toe, the inner. When young are born to such a parent or parents, (for it matters not whether one or both of the parents have been operated upon) they have this peculiarity of having only one toe on the posterior foot. Sometimes, however, they have additional toes, which in this case are attached by a pedicle to the limb.

Now, all of those peculiarities which have been observed in the parents, all things in all their details, are witnessed also in the Guinea-pigs, hereditarily born toeless, who have developed epileptic phenomena. There is, therefore, an inheritance of a power to develop the disease, but no inheritance of the disease itself. Dr. Dupuy has examined the sciatic nerve of such animals, and found them healthy, before, during, and after the

existence of the disease. He has also followed these experiments through five generations.

Dr. Dupuy made allusion to the doctrine of Balbiani on embryogeny, and thinks that, according to Balbiani's laws, the phenomena of inheritance, in the case of epilepsy, can be explained, epilepsy being a malady of nutrition like all other nervous diseases. Dr. Dupuy stated that only those young which are born with alteration in the normal nutrition of parts become epileptic; in such the disease fatally occurs.

The paper being open for discussion.

DR. JEWELL. Did you say that those animals upon which you had operated invariably transmitted their deformity? Dr. DUPUY. No not invariably, but it is so sometimes.

DR. HAMMOND. I have been astonished at the small proportion of transmitted cases of epilepsy in the human species. Although its hereditary nature is undisputed, yet I do not think it is by any means so very common as is generally supposed.

DR. LORING. I would like to ask in regard to the defect in the toes. I would like to ask, whether in following out five generations those two toes were absent, or whether the tendency was towards the normal number? We know that in the case of the Chinese women, no matter how badly the feet are deformed, no deformity is found in the feet of their children. I ask this question as bearing upon my own specialty, in regard to the inheritance of diseases of the eye: it has never been proven that any hereditary tendency has been produced, and I would like to know Dr. Dupuy's experience? Also if by simply cutting off the toes the defect would be transmitted? And in the case of circumcision, whether there is any alteration in the prepuce?

DR. DUPUY. I thought that I said that such deformities were very rare. It appears that to have a lesion transmitted it must cause an alteration in nutrition. The tendency was of course towards the normal number of toes, since such a small number of the young have the deformity.

Last year I read a paper in the "International Ophthalmological Congress," which met in this city, in which I showed that the phenomena followed a lesion of the sympha-

thetic nerve, or the corpora restiformia, in the Guinea-pig, are invariably inherited by their young, and it is known that these lesions modify the organs of sight.

In reply to Dr. Loring's question as to whether the simple act of cutting off the toes would be inherited, I would say, I have not tried that, but it is known that in dogs, when their tails have been cut off, sometimes their young are born without tails. In cutting off the toes of the Guinea-pig the lesion is so small as not to cause any disorder of the nervous system. As regards the Chinese and circumcision, we know nothing positive in regard to those, as no body has ever made examinations, it is impossible to say whether there is any change in those parts. I must say that that argument has been constantly put forward. As for fingers I know a lady in England, who lost her ring finger and little finger, and sometime afterward gave birth to a female child, in whom the same fingers were absent.

DR. LORING. Do you know of any cases where deformities produced by amputation, have been transmitted? Dr. Dupuy. I know of two cases where amputation of the feet have been transmitted.

DR. SEGUIN. It strikes me, Mr. President, in regard to the question put by Dr. Loring, that we must take into consideration the fact that the human subject presents much more resistance to these influences than the lower animals; still Dr. Dupuy's facts are of importance, because by their aid we can establish an argument by analogy with proper restrictions.

DR. SPITZKA. I have read in Obersteiner's essay, that those animals which are most likely to become epileptic, are those which resemble their parents in color or hair. We know that in insanity the chances of transmission are greatest when the resemblance is greatest. The experiments of Westphal led to the view that epilepsy depended upon hemorrhage in the medulla oblongata.

DR. DUPUY. The experiments of Westphal are very rough and brutal; he used to knock the heads of his Guinea-pigs against some hard object and of course extravasation of blood took place, not only in the medulla oblongata but also in other parts of the centres. In the Guinea-pig there is a point at

the base of the brain, the mere pricking of which, induces an immediate attack of epilepsy. Now it may be that Westphal, in his experiments, knocks the pigs in this place and an attack of epilepsy ensues. I have had over one thousand Guinea-pigs under observation, and I have only noticed transmission seven times. I do not lay much stress upon the results of Obersteiner, because his results show transmitted epilepsy too often.

DR. SPITZKA. I simply repeated what I had read in Obersteiner. As regards the *modus operandi* of Westphal's experiments, whether they are brutal is not the question, but whether they do cause epilepsy. About seven per cent. of all human cases of epilepsy are traumatic. I think there is no doubt but what in Westphal's experiments the cases were epileptic. In regard to the third point or Dr. Dupuy's stating that the cell of Balbiani is more important than the cell of Purkinje, I do not admit that.

DR. DUPUY. As for peripheral lesions causing epilepsy, I can make no ratio, although cases are not rare. In regard to Balbiani's cell, I would say that all biologists have accepted his views, moreover, I beg leave to state that it is taught, that Balbiani's cell and Purkinje's cell are two different things. The first carries ancestral tendencies and the second, maternal; that is based upon the history of the development of those bodies; moreover they fecundate one another and have an independent existence: they afterward retrograde if male fecundation does not supervene.

The germinal vesicle is now determined by embryologists, especially Oellacher and His to have no relation to embryonic development whatever, as soon as fecundation takes place it is driven to the periphery of the yolk, collapses, and becomes an inert body. The collapse is a process of retrogression which only supervenes if no male fecundation occurs.

DR. MASON. I have seen Prof. Westphal perform his experiments, and I think others place more stress upon his experiments than he does himself. From what he has said to me, I think he looks upon the experiment more as a joke than anything else.

DR. JEWELL. There is one point I think, which is left out

of consideration by Dr. Loring, in his remarks about the feet of Chinese females and similar cases: no deformity is transmitted except where some of the parts have been removed. A distinction should be made between those changes which are merely morphological and those which are structural.

DR. DUPUY. The Bretons have been in the habit of putting out of shape the heads of their children, for ages, as shown by fossil crania and they practice the same up to this date, yet their heads are not congenitally deformed.

DR. JEWELL. In considering the influence which the nervous system may have upon intra-uterine peripheral changes, especially where there is loss of members, it should be remembered that the peripheral nervous system makes its appearance first, and hence before the spinal cord could produce any influence upon the members. This fact has occurred to me as of considerable importance, especially in the case of those monsters which a perfect peripheral nervous system, and the spinal cord and brain undeveloped. That these peripheral parts have taken their shape, before the central nervous apparatus can exert any influence, is a point which should be taken into question in considering changes which take place *in utero*, that is before the central nervous system has been developed sufficiently to enable it to exert an influence on non-nervous parts.

#### CASE OF LOCOMOTOR ATAXIA IN A CHILD.

DR. J. C. SHAW, of Brooklyn, exhibited a very interesting case of locomotor ataxia in a female child four years of age. When eighteen months old she had measles and scarlatina, and in a few months afterward she began to experience difficulty in walking, followed by an inability to hold things in her hands. This difficulty increased until she was unable to walk. She now presents marked ataxic movements in both of the upper and lower extremities. When the child was supported, and she attempted to walk, both of the lower extremities were jerked forward and outward, and the heel brought down first, in a truly ataxic manner. There did not appear to be any anæsthesia. It had been observed that at times the child would scream out without apparent cause. On May 2, 1877, she screamed out suddenly and said that she

had a pain in her left heel. She has no muscular atrophy or paralysis, and the muscles react normally to faradization.

Dr. Shaw stated that the diagnosis was made by exclusion, and gave his reasons for excluding, brain tumor, lesion of the ganglionic cells of the anterior horns, paralysis from peripheral irritation, and pseudo-hypertrophic paralysis; and advanced an argument to show that it was a case of sclerosis of the spinal cord—disseminated, perhaps, and located in the lumbar part of the cord, and in the posterior columns.

REMARKS UPON DR. SHAW'S CASE.

Dr. SEGUIN. I am not aware of any cases of locomotor ataxia younger than twenty years, except Friedrich's cases, which were probably cases of disseminated sclerosis.

Dr. HAMMOND. I think it a little doubtful in regard to existing ataxia in the upper extremities of this patient. The defective vision is competent to account for the motorial disturbance observed.

Dr. SHAW. I would like to ask Dr. Loring if it is possible, from the appearance of an atrophied optic nerve, to say whether there had been choked disk at any time?

Dr. LORING. It is often very difficult to determine in a given case of atrophy of the optic nerve, whether this has been preceded by choked disk or not. There are, however, some ophthalmoscopic signs which are, if present, of some assistance in determining this question. With choked disk there is usually a considerable amount of swelling of the nerve, together with a large amount of hypertrophy of the connective tissue, which after the active stage of the inflammation has subsided can be traced, as fine bands along the vessels, not only in the head of the nerve, but also into the adjacent tissue of the retina. Moreover, in atrophy from choked disk the edge of the nerve, instead of being sharply cut, is more or less indistinct, has a ragged appearance, its contour being dotted here and there with the remains of choroidal pigment. From what I can judge, that little child has not much interference with vision. I would ask Dr. Shaw, if there was any evidence of a limited field of vision?

Dr. SHAW. Judging from the child's actions, I should think there was no limitation to the visual field.



DR. HAMMOND.—The youngest case of ataxia that I have ever seen, was 25 years of age.

The next was a paper read by Dr. William A. Hammond, of New York, which was, as Dr. Jewell afterwards remarked, interesting from a scientific, as well as from a theological point of view. The title of the paper was,

THE ODOR OF THE HUMAN BODY AS DEVELOPED BY CERTAIN AFFECTIONS OF THE NERVOUS SYSTEM.

DR. HAMMOND called the attention to some facts in regard to the natural odor of the body in the human species, and of the faculty which some of the lower animals possessed,—that of differentiating between the odors of different individuals. Besides the inherent odor of the body there was reason for believing that an entirely different one may be given off, not only as a consequence of disease, but as a result of emotional disturbance. During the middle ages, manifestations of the kind in question were not uncommon in the persons of both sexes, and were attributed to miraculous power. That such cases existed was probable, not, however, as a special gift of God, but as a neurosis similar to other instances which had come under Doctor Hammond's own observation. Cases were then cited, of a number of the more important instances among the saints, who were considered highly odoriferous. So far as the author of the paper was aware, there had been no attention given to the subject in the relations now under notice. The cases cited by Dr. Hammond as bearing upon this point were briefly as follows:

A young married lady of strong hysterical tendencies, from whom, during a paroxysm, an agreeable odor, similar to that of violets, was exhaled only from the left lateral half of the anterior wall of the chest. At such times the perspiration was remarkably increased in this region, as compared with the corresponding part opposite. The odor was perceptible at a distance of several feet, but was entirely absent during the intervals of the paroxysms. From an examination of an alcoholic extract of the odoriferous perspiration exhaled by this patient, it was presumed that the odor was due to the presence of butyric ether. The local application of several remedies to the parts, among which were preparations of carbolic acid,

soap and water and other alkaline substances, gave the patient only temporary relief from the odor; but the internal administration of the salicylate of soda, in doses of five grains, entirely cured this lady of her violaceous odor, and the perspiration of the region was reduced to the normal character.

A second case was that of a young lady in whom the first exhibition of the odor (in this case, that of pine-apple) occurred contemporaneously with an attack of chorea.

In a third case, a pine-apple odor was emitted from the skin of the head, neck, and chest of a woman whenever she was angry.

A fourth case was that of a man who, during frequent hypochondriacal periods, emitted a violaceous odor. Occasionally cases were met with from whom a disagreeable odor was exhaled during sexual excitement. No opinion, as to the actual and immediate cause of these odorous emanations, was expressed, further than that they were due to a nervous disturbance.

DR. HAMMOND passed around a small vial containing an alcoholic extract of the odoriferous perspiration of his first patient, which had a distinct violet smell; also a second vial of the same extract, with the addition of bicarbonate of soda, smelling strongly of pine-apple.

DR. BEARD.—I was very much interested in Dr. Hammond's paper, and especially with the treatment. I have lately had a case where there was (a profuse) odor from the armpits. A case of profuse perspiration in the hands under care of Dr. Sterling, was cured by injections of atropia. Speaking of those historical cases mentioned by Dr. Hammond, I do not know that you can call them facts. It is said that Alexander the Great had a pleasant odor about his person; but there is much of fallible human testimony in regard to all such claims.

DR. JEWELL.—I think one thing can be said in regard to Dr. Hammond's paper, and that is, it has furnished a new means of disposing of the phrase "odor of sanctity." Believing as I do that the secretions are largely under the control of the nervous system, I do not see why matter from the skin may not acquire certain odors, from a nervous causes. The subject has undoubtedly not secured the attention that it ought to have.

DR. HAMMOND.—Mr. President, Dr. Wright informs me that he found in the case of a woman, the odor of asparagus, not only in the urine but in the perspiration also.

DR. SEGUIN.—I was told by Dr. Brown-Séquard, that the wife of a French physician, emitted a strong general odor after sexual intercourse, and this led to the detection of her adultery.

DR. MCBRIDE.—Dr. Priestmann, of Nicolaëff, reports in the *Med. Cent. Zeitung*, No. 2, 1877, that, for about six hours after coitus, a peculiar odor is discernible in the breath. In support of this, he cites the case of a man arrested for alleged rape, two hours after, it was stated to have taken place. On account of the absence of the characteristic odor, Dr. Priestman swore positively that the accused had not had connection within six hours, and further investigation confirmed this opinion.

Dr. Weir Mitchell reports in the *Am. Jour. Med. Sciences*, July, 1873, that a curious odor of the breath is perceptible in many cases of meningitis.

DR. SEGUIN. My father is a believer in the possibility of diagnosing certain diseases by smells. Sir William Gull, in the Pathological Society of London, has expressed the opinion that syphilitic subjects have a peculiar odor.

DR. HAMILTON cited the case of one gentleman, an Inspector of the New York Board of Health, who could go into the lower hall of one of the large tenement houses, and tell, by sense of smell, whether there was a case of small-pox in the house or not.

DR. JEWELL. There is one disease, namely, *milk sickness*, in which the odor is very remarkable; any one who has ever smelled it will never forget it. Of course it is impossible to describe it, unless there is some other odor like it, with which we are all familiar.

In regard to these odors, a great deal will depend, I think, whether they be general or localized, as for instance, an odor, not from the armpits, but from some unaccustomed part of the body, then the presumption will be strongly in favor of a nervous disorder, but if general, it may probably have no relation to the nervous system.

DR. SPITZKA. In measles there is an odor like that of freshly picked geese.

DR. HAMMOND. In answer to President Jewell's remarks, I would say that the odor was limited in the case I have mentioned, to the left lateral anterior part of the chest.

After the close of the discussion, Dr. GEORGE M. BEARD, of New York, proceeded to read a paper on

THE ENDEMIC TETANUS OF EASTERN LONG ISLAND.

That tetanus had been more frequent in portions of Suffolk county, Long Island, than in other parts of the country, was well known. Dr. Beard had passed several of his summers there, had conversed with a number of the physicians and residents, and corresponded with nearly all of the physicians of the county, asking for facts rather than opinions.

Dr. Beard's conclusions up to statement are as follows:

1. For the last three-quarters of a century there has been an endemic tetanus in certain portions of Suffolk county. It consists of both the traumatic and spontaneous varieties, and affects animals as well as man.

2. This endemic abounds mostly in the towns of the south side, especially at the Hamptons, is less common in the central towns, and on the north side, and in Montauk does not exist.

3. The endemic has been on the decline for the past ten or fifteen years, and in the central portion (excepting Riverhead) no longer exists.

For the causation there are three conceivable theories,—geology, the use of fish on the land as manure, and dampness in the air.

The theory based upon geology and advocated by Dr. B. D. Carpenter, who has studied the subject, appears to be disproved by the decline in recent years. Geological conditions are constant factors. In favor of the fish-on-the-land theory is the occurrence of the disease in those localities where fish are most used, and the decline of the disease with the decline in the use of fresh fish as manure. Dr. Beard thinks, however, that the facts up to date seem to favor the theory of dampness in the air combined with the local dampness of the soil.

Dr. Beard would treat a case of tetanus by calabar bean

(English preparation), in small doses every hour or half hour, so as to affect the pupil, and at the same time apply ice-bags to the spine. As suggested by Dr. Carpenter, the patient should be kept absolutely quiet if possible. The local application of the oil of turpentine to all wounds, is practiced by a number of the physicians of Suffolk county, and is to be recommended. The apprehension, felt by New York surgeons that tetanus is likely to follow surgical operations in Suffolk county, is not justified by the facts as they now stand.

There were no remarks on this paper.

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#### SECOND DAY--EVENING MEETING.

The Association was called to order by the President at 8 P. M.

Present: Drs. Jewell, Miles, Hammond, Rockwell, Emerson, Spitzka, Shaw, Beard, Gibney, Cross, Kinnicutt, Loring, Mason, McBride, Dupuy, and Seguin.

The first paper of the evening was read by Dr. A. D. ROCKWELL, which consisted of the history of a remarkable case of

#### INTERMITTENT HEMIPLEGIA.

A brief outline of the case is as follows: A stair-builder, aged forty-nine, in fair health, was seized one afternoon in July with dizziness, loss of speech, and complete paralysis of the left side, which lasted for twenty minutes, and then entirely disappeared. Similar attacks occurred every other day for three weeks thereafter, at about the same time of the day. One day about the middle of August he had a much severer attack at 11 A. M., which was repeated every day at the same hour until September 3d, and between 11 A. M. and 4 P. M. each day, during all this time, paroxysms re-occurred three or four times. In the more severe attacks, he was unable to walk or speak, but during the milder ones he could move with difficulty and speak indistinctly. Dr. Rockwell's treatment consisted of a mild *séance* of general Faradization, in conjunction with three two-grain doses of quinine daily. There was decided improvement

at once, and on the 25th of September, he was discharged as apparently cured. He remained so until December 4th, at which time the paroxysms re-occurred, and in a more violent form, and he died on the following day.

Post-mortem examination showed congestion of the surface of the brain; pia mater covered with a thin film of organized lymph from old inflammations; texture of the brain softer than normal; choroid plexus enlarged and cystic; basilar artery and part of the circle of Willis enlarged and atheromatous; mitral valves and liver in a condition of fatty degeneration; serous effusion at the base of the brain, but there were no arteries ruptured, no evidences of embolism or thrombosis.

As the above described pathological changes seemed hardly sufficient to account for the unusual symptoms and suddenness of death, Dr. Rockwell discussed at some length, the probable cause of the final result, and, by means of exclusion, thought it probable that death was due to a spasm of the vessels in the brain tissue, which rendered it unfit to discharge its proper function.

DR. EMERSON. I would like to ask Dr. Rockwell, if he made observations on the temperature of his patient?

DR. ROCKWELL. No, sir; I made no record of the temperature during treatment, but the temperature in the shop on the occasion of the fatal attack was found to be 105 degrees.

DR. SHAW. I would like to ask, if there was any syphilitic tendency?

DR. ROCKWELL. There was none.

DR. HAMMOND. Were the attacks on the same side?

DR. ROCKWELL. They were all on the same side.

DR. CROSS. Were all of the arteries at the base of the brain examined?

DR. ROCKWELL. I think none were overlooked.

DR. HAMMOND. I am glad to hear the details of this case, for I have a patient under treatment (who presents similar phenomena): three weeks ago my patient was attacked with paralysis on the right side, did not lose consciousness, but called for assistance and went home; very soon afterward he had a second attack; he had three, four or five in the course of that afternoon, and the next day I was called in consul-

tation. Upon my arrival, I found paralysis on the right side; there was hypertrophy with dilatation of the heart. While I was at his bedside, he remarked to me that he was going to have another attack; he had at least three or four while I was there, which was about an hour; I then did not have an opportunity to examine with the ophthalmoscope; there was *arcus senilis* on both sides. He was treated with infusion of digitalis. He has kept up and has had no further attack. He has now, this afternoon, distinct hemiplegia on the right side. His speech is pretty good, his mind is a little enfeebled, and he can only walk by holding on to some one's arm. The case is, I think, similar to Dr. Rockwell's. I mentioned the case to Dr. Jewell, and he suggested at once the existence of capillary thromboses.

I think that in this case hemorrhage must be excluded entirely, as also in the case reported this afternoon. It seems to me that Dr. Rockwell's case and mine are probably analogous. The eye-sight of my patient is perfectly good.

DR. SPITZKA. Of course, in the absence of *post-mortem* evidences, we are limited to conjecture, and, as far as I can gather evidence, there are three things which can cause this. One is epilepsy; a second is partial thrombosis of the cerebral arteries; third, spasm of the cerebral arteries. The doctor then cited two cases found in the insane asylum of Vienna. Dr. Spitzka expressed the opinion that ideas on thrombosis were a little biased at present, and that many conditions of capillary and arterial stasis, which deserved the name thrombosis, were not so regarded.

DR. BEARD. I had a case of intermittent hemiplegia in a patient 35 years of age; he recovered and is well to-day. I never saw him in an attack. The attack would last from a few minutes to half an hour. The trouble lasted one or two years. I made a diagnosis by exclusion of spasm of the arteries, and thought that he would get well after taking a course of electrical and other treatment. The prognosis, if not the diagnosis, was confirmed.

Two years ago a gentleman consulted me for a feeling of numbness in the right, upper, and lower extremities, which had existed in an intermittent form, he declared, for several

days. While present in my office he had several distinct partial hemiplegia attacks, each of a few seconds duration, during which there was a decided loss of power in the right upper and lower extremities. Later in the same day, he had several well marked aphasic attacks of very temporary duration, however. Twelve hours later there was developed a complete right hemiplegia, with loss of consciousness. Specific trouble being suspected, very large doses of iodide of potassium were given, and with the most satisfactory results, the hemiplegic symptoms with the aphasia partially yielding within forty-eight hours after the administration of the first dose. Under continuous treatment, the patient so far recovered as to be able to be about, at which time he passed from my observation. A temporary spasm of a cerebral vessel or vessels suggested itself at the explanation of the symptoms preceding the development of the complete hemiplegia.

DR. SPITZKA. My explanation was only for those cases which were not syphilitic; I suppose your case was syphilitic. Your case I would explain as due to a thickening of the vessels,—syphilitic neoplasms. In your case I would suggest that the inner coats of the arteries were roughened, and thrombosis took place with great rapidity.

DR. DUPUY. I am exceedingly pleased with the view put forward by President Jewell. It is contrary to the statement of Prof. Charcot, whose theories have been so widely noticed by Dr. Jewell. I do not believe that the cortex has anything to do with the nature and the production of these phenomena in a direct manner. As for the re-establishment of collateral circulation, I must say that I disagree with our President; it never takes place on account of the peculiarity of the cortex circulation.

DR. SPITZKA. Did I understand you to say that the cortex of the brain had anything to do with the movements?

DR. JEWELL. I did not wish to be understood that it has to do *directly* with muscular motion; my opinion is, that while it does not form part of the motor apparatus proper, that the cortex is a part from which excitatory impressions do start, to be projected on the motor apparatus below.

There was no further discussion.



The next paper was read by Dr. V. P. GIBNEY, of New York, and entitled

SPINAL IRRITATION IN CHILDREN.

The paper consisted of the clinical histories, together with remarks upon a number of cases which Dr. Gibney was wont to consider as cases of spinal irritation.

In the course of the lengthy and animated discussion which followed it was made evident, that there was some difference of opinion as to what spinal irritation really was, several of the members of the Association having expressed their disagreement with Dr. Gibney on this point. The question of local mal-nutrition was raised and freely discussed.

Remarks upon Dr. GIBNEY's paper, viz.:

Dr. JEWELL. I will make a remark or two and then give way to others. I was much interested in the paper which has just been read, but many of the cases cited were not what I call spinal irritation; they are rather cases of spondylitis, that is inflammation of the joints between the vertebræ, the inflammation extending subsequently to the dura mater and sometimes passing into effect the cord itself. They seem to be more truly cases of spondylitis—leading to muscular and sensory troubles as a consequence of the disease, not of the spinal cord but of the dura mater. Seventy or eighty such cases are reported by Braun, in a recent monograph. The cases cited in the last paper are not, many of them, such as I am accustomed to call spinal irritation. It is important to define just what one means by the terms employed.

Dr. HAMMOND.—Mr. President, I am very much interested in the paper of Dr. Gibney, but I think the name is unfortunate because the cases are not such as are usually considered cases of spinal irritation. Indeed they have scarcely any analogy with these latter, except in the one respect of local tenderness. This, though perhaps, an essential feature of spinal irritation is, of course met with in various other diseases either of the vertebral column or its contents. I am very reluctant, Mr. President, to open up any discussion on a subject about which there is so much difference of opinion as exists in regard to spinal irritation. Such a discussion would, in my

opinion, be out of place for the name given that Dr. Gibney's cases are not instances of that affection.

DR. DUPUY.—Perhaps it will be a good plan to give a typical case of spinal irritation? There is not a fact in experimental science to support the view that pain is due to anaemia of the nervous centres. On the contrary, experiments show that pain is found to be present together with hyperaemia of the centres as proven by the experiments of Brown-Séquard on the spinal cord, and those made by the pupils of Ludwig in Leipzig, I refer to the hyperaesthesia following lesions of the spinal cord, and other experiments.

DR. HAMMOND.—I am not going to attempt to give the description of a typical case of spinal irritation, for there is no such thing. One might as well attempt to give a typical case of constitutional syphilis. As to the symptoms of spinal irritation being due to organic lesions, I think that is absurd; for I have seen all the symptoms disappear with a single application of galvanism to return the next day perhaps, and again to be dissipated by like means.

DR. DUPUY.—I do not see where the distinction lies. Duchenne de Boulogne has shown how to stop the most excruciating pains in the heads of patients suffering from tumor, sclerosis, or other disorders, by a mere application of electricity; also later, Dr. Charcot has shown the same; so that fact which Dr. Hammond brings forward does not show that pain is due to anaemia of the cord and not to organic changes in the membranes of the cord, or other organic changes as Dr. Gibney shows; because the pain can be removed by galvanism—for the same obtains with organic diseases.

DR. BEARD.—I did not suppose that there was any difference of opinion among neurologists in regard to the general nature of spinal irritation. It is spinal tenderness with various attendant symptoms in which every symptom of inflammation in organic disease is excluded.

DR. HAMMOND.—One other point: I have known, at least, a dozen cases where patients have been cured of every symptom present—neuralgic pains, vomiting, spinal tenderness, muscular contractions—all disappearing on a single application of the actual cautery, and I have known all of these phenomena

to reappear upon the occurrence of emotional excitement, which I think is entirely inconsistent with the theory of any organic lesion.

DR. DUPUY.—The same will happen when organic lesions are present as I have just said.

DR. JEWELL.—I recognize two classes of cases which may be named spinal irritation; one in which there is distinct spondylitis, if you please, and possibly inflammation of the dura and of the spinal cord. But there is another class of cases which I cannot allow to go into the one just named. I may illustrate what I mean by a case like the following. A gentleman came to consult me, who was accustomed after certain meals, when unwell, to have his food apparently undergo a species of fermentation. Attempts at regurgitation of food were unsuccessful, on account of spasm of the oesophagus. In this case, coincidently with the gastric disorder, there was severe pain, and marked spinal tenderness about the fifth, sixth and seventh dorsal vertebra. Pain also was experienced in the shoulders, and down the arms. But as soon as his stomach was in a better condition, tenderness would be entirely gone. Now, in this case there was what I call spinal irritation,—there was intense hyperalgesia, not hyperæsthesia, for I think they ought to be distinguished from each other. I do not think, it more than just possible, that the case I have described was due to spondylitis. With regard to the explanation of this class of cases, the point I wish to make is this; that they do not depend on so serious a lesion as a local inflammation would be, but that they depend rather on a combined nutritive and circulatory lesion of the cord which had not only been produced by peripheral irritation of related nerves, but in turn may induce it elsewhere.

DR. DUPUY.—I do not wish to be understood as saying that spinal irritation is spondylitis: All I want to say is, that it is not due to anæmia of the cord, but is the result of irritation inducing alteration in the nutrition of that centre by an augmentation of the blood supply. The fact that the pain can be relieved by electricity and that it reappears, does not show that pain is not due to an organic disorder.

DR. HAMMOND.—But there is much more in spinal irritation

than pain. And as to pain not being a consequence of anæmia, I would like to ask Dr. Dupuy, if he recognizes the existence of such an affection as anæmic headache.

DR. DUPUY.—Yes sir, I do; but the cases we have considered to-night have no connection with that. Anæmic persons are prone to congestion of certain organs, and that induces the headache, for instance, costiveness, dysmenorrhœa, anorexia, *et cætera*.

DR. HAMMOND.—I think they do have some analogy with anæmic headache, making allowance, of course, for the different anatomical situation of the disturbance. But as I said, I am not going to discuss the subject of spinal irritation. At most, our views of its pathology are but guesses, and my guess is that it is essentially anæmic. My reasons for this opinion have been given in full in other places.

DR. SEGUIN.—Before speaking of what I think spinal irritation is, I must coincide with Dr. Jewell, that the cases related in the paper are not all such as I have been in the habit of calling spinal irritation. In regard to the pathology of spinal irritation, I have no theory to which I cling with any force. If there is one theory that I feel like accepting more than another, it is the one which I have for several years taught in this amphitheatre, namely, that spinal irritation is due to a malnutrition of the nervous centres, not directly to anæmia or hyperæmia. I am not prepared to accept the anæmic theory. I agree with Dr. Jewell, that many cases of spinal irritation are symptomatic. I know of many cases of spinal irritation, which were cured by proper treatment of the local trouble. The treatment of urethral irritation, I have seen relieve spinal irritation. Still I do not deny that malnutrition of the spinal cord, may give rise to symptoms located in the genitals, or in the uterus, or in any other organs.

DR. DUPUY.—Malnutrition of the spinal cord is nothing but hyperæmia brought on by reflex action.

DR. GIBNEY.—There is one case I did not report; it was a case of spinal irritation, typical, as far as I know. It occurred in a girl ten years of age. There was parotitis and much irritation over the region of the ovaries. I was called up several times to see her, they thinking that she was dying. She

had the typical eruption of typhoid fever on one or two occasions, but there was no elevation of temperature. She was in this condition for two or three months. One day in examining her, I passed my hand over towards the spinal lumbar region, and found that there was excessive tenderness. This was treated, and in two or three weeks she recovered, and has had no irritative symptoms since. At one time I thought she had typhoid fever, but the temperature saved me from making that diagnosis.

I had three or four cases of unmistakable spondylitis, but the other cases I am sure were not spondylitis.

DR. CROSS.—I would like to ask Dr. Jewell how he accounts for the malnutrition of the spinal cord, if there be neither anæmia or congestion. For the cord to be in that abnormal condition there must be, according to my idea, either anæmia or congestion, if we, perhaps, except cases of shock. Consequently, malnutrition must depend upon one of these two conditions. The symptoms are not those of congestion, and the influence of the strychnia treatment goes to prove that fact. If then there be no congestion and no anæmia, I would like to have him explain to me how there can be defective nutrition.

DR. JEWELL.—The malnutrition arises, either from a defective supply of nutritive materials, as in case of impoverished blood, or from too prolonged action or excitation—too little rest of the cord, or in both these ways combined. There is thus a more or less marked tissue waste produced, and irritation sets in, and without any circulatory disorder necessarily being present. Just as surely as you put these patients at rest they get better; for, if they are not kept quiet, tissue waste so exceeds tissue repair as to prevent recovery.

DR. HAMMOND. I would like to ask Dr. Jewell, how he can explain the fact of the existence of irritation in the lumbar region of the cord, produced by depraved blood, at the same time that good blood is furnished to other parts? I cannot conceive of good blood going to one place and bad blood to another.

DR. JEWELL. That is not the point, doctor. It is, that the part in which a change has been excited, has more work to do

than it should do, or than other parts do, or relatively induces more excitation. The blood may be as good for that part as other parts of the body, but that part sustains relatively excessive tissue waste as compared with others.

DR. HAMMOND. I cannot conceive of local malnutrition, independent of local lesion.

DR. CROSS. That was the point I put. I merely asked the question for information. I do not see how your argument stands on any better basis, for may not anaemia produce malnutrition just as well?

DR. JEWELL. Most certainly; that is one way in which it is produced. I am not arguing against anaemia, but that I think there are cases in which it does not occur. The blood may not have anything to do with them. They may be solely due to too much activity, and too little rest. It is not malnutrition, therefore, so much as denutrition (excessive waste) which occurs in these cases.

DR. DUPUY. It has been proven by Brown-Séquard, twenty years ago, that there is no pain produced in the spinal cord if there is no increase in the vascularity of the gray matter.

DR. SEGUIN. I would like to ask Dr. Dupuy whether Dr. Brown-Séquard reached this conclusion by inferences from experiments, or whether he proved its truth directly by experiments?

DR. DUPUY. He has directly proven them, and so have the pupils of Ludwig. The experiments he has published. He has shown that when the spinal cord is laid bare, if it is protected from the atmosphere, there is no hyperaemia, and consequently, no hyperaesthesia. The experiments of the pupils of Ludwig, made a few years ago, show conclusively that hyperaesthesia is due to vaso-motor paralysis.

DR. SEGUIN. The experiments are very complicated, and there are many sources of error in them.

At the close of the discussion, Dr. E. C. SEGUIN read a paper entitled

A CLINICAL CONTRIBUTION TO THE STUDY OF POST-HEMIPLEGIC CHOREA.

He related the history of two cases. In the first, observed in 1873, a young man of eighteen suddenly became paralyzed

in the left arm, without disorder of sensation. Later, some numbness appeared in the left hand, and in about two months convulsive movements manifested themselves in the left upper extremity. About this time the left leg became affected with weakness and tremor; later still, the lower part of the left side of face twitched. The arm and leg were the seat of convulsive movements of choreiform type, made worse by emotion or attempt to use parts; no ataxia; sensibility slightly lessened in hand. Later, there occurred palsy of third cerebral nerve on the right side; palsy and spasm in left face; he died, but autopsy could not be obtained.

Case second was presented to the Association. Its history was briefly this: Male, aged twenty-six years; in April, 1876, sudden right hemiplegia, with partial loss of consciousness and temporary aphasia. During second and third months, convulsive movements appeared in the right arm and hand, while great recovery of muscular power was obtained. Partial right temporal hemiopia. Numbness and slight anæsthesia in face, arm, and leg on right side. A few epileptiform attacks since. Seven years ago had chancres, followed by doubtful secondary symptoms. Movements on right side are now nearly normal in force; while at perfect rest slight oscillations (of paralysis agitans type) occur in fingers and hand. During attempt to use hand violent inco-ordinate movements occur, irregular extension and flexion, of quasi choreic, quasi ataxiform type. Partial right hemiopia and hemianæsthesia.

In both these cases the movements, first described by Dr. S. Weir Mitchell are present; and from the other symptoms present it would appear as if the lesion in these cases had been in the basal part of the hemisphere, just below and posterior to the thalamus opticus, involving, to a certain extent, the internal capsule. In the first place, the lesion extended downward so as to involve the crus or the origin of the third nerve.

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WEDNESDAY, JUNE 8TH.—AFTERNOON MEETING.

The Association was called to order at 2 p. m.

Present: Drs. Jewell, Miles, Shaw, Hammond, Dupuy,

Loring, McBride, Cross, Beard, Kinnicutt, Emerson and Seguin.

The Committee on Nominations presented the following report, through DR. MILES, its chairman:

*For President*—Dr. J. S. Jewell, of Chicago.

*For Vice-Presidents*—Dr. F. T. Miles, of Baltimore, and Dr. S. G. Webber, of Boston.

*For Corresponding Secretary*—Dr. J. J. Mason, of New York.

*For Recording Secretary and Treasurer*—Dr. E. C. Seguin, of New York.

*For Curator*—Dr. T. A. McBride, of New York.

On motion, the report of the committee was accepted, and the gentlemen named duly elected officers of the Association for the ensuing year.

Under the head of Miscellaneous Business, the President read a letter from Dr. William A. Hammond, offering to the American Neurological Association a prize of two hundred and fifty dollars to be awarded at its next annual session on the favorable report of a committee of three of its members, to the author of the best essay on the anatomical and physiological effects of strychnia on the brain, spinal cord, and the nerves.

COMMITTEE ON PRIZE ESSAY.

Dr. S. Weir Mitchell, of Philadelphia; Dr. J. S. Jewell, of Chicago; and Dr. E. C. Seguin, of New York.

DR. MILES exhibited a cast of the brain, showing an exceedingly rare anomaly in the arrangement of the cerebral convolutions. On the left side of the brain, the fissure of Rolando was bridged over by a complete secondary gyrus. This, Dr. Miles thought, had been seen but once or twice.

The reading of papers being next in order, DR. J. C. SHAW read a short paper entitled,

A CONTRIBUTION TO THE SYMPTOMATOLOGY OF BRAIN TUMOR.

Given in brief the two cases cited were as follows:

A lady, aged fifty; "choked disk," both eyes; dizziness, headache, attack of falling, without convulsions general or local; no paralysis. Post-mortem examination revealed a sarcomatous tumor in the interior of the orbit on the right side, pressing upon the inferior and middle frontal convolutions.



W. P., aged twenty-nine; male; "choked disk," attack of falling without loss of consciousness; no paralysis, choked disk lasting three and a half years without passing to atrophy; vision  $\frac{3}{8}$ . Post-mortem—Pedunculated cystic tumor pressing on the anterior surface of the left temporo-sphenoidal lobe. This man was one of the victims of the fire in the Brooklyn Theatre.

DR. SEGUIN. I never accept the statement of a patient in regard to loss of consciousness, when the patient falls down. That there was no paralysis in the second case is in accordance with my own experience. I have observed a case of tumor larger than a hen's egg springing from the dura mater and from the squamous portion of the temporal bone, violently compressing the temporo-sphenoidal lobe. There were only three epileptiform attacks, never any paralysis, no choked disk, and the patient died in mixed tonic and clonic convulsions. I was lead to localize the lesion correctly in the right hemisphere by finding vaso-motor paralysis on the left side of the body as shown by a red patch on the buttock, and higher temperature.

DR. MILES. In a case which I had, after death the tumor was found to be of considerable size situated in the portion of the brain, regarded as not motor. There was no loss of power, no paralysis, and, if it does not appear flippant, I will say how I found it out. I had not seen the patient, but I asked the question, and it was answered in the affirmative, if he had used the chamber vessel? So I thought there was no paralysis of the legs.

DR. DUPUY. It is well known that persons suffering from paralysis, due to softening of the brain, will sometimes get out of bed and pass water, on the bed of their neighbors—or do other mischiefs.

DR. MILES.—I think, that does not have anything to do with my case.

DR. DUPUY. It shows that paralysis can follow or not follow, no matter what territory of the brain is diseased.

#### VASO-MOTOR CENTRES.

DR. EUGENE DUPUY made a communication to the Association on the above subject, which was to the effect that vaso-motor centres are not to be looked for in the spinal cord or

brain, but outside of them. The vaso-motor centres are made up of the ganglionic system and of exceedingly numerous ganglionic cells, that are to be found scattered in the pia mater in the neighborhood of the medulla oblongata and the pons; also in the pia mater of the anterior parietal portion of the brain; also from ganglia that are found on the track of the ganglia of the trigemini, and the ganglia of the spinal and cerebral nerves; also from the ganglia that are scattered through the cranial and other viscera.

That vaso-motor fibres only become connected with the cerebro-spinal system at their apparent origin, that is to say, at that point where the nerves get their sheath of pia mater after leaving their centres.

DR. JEWELL. This is a subject in which I have felt much interest. I have formed certain notions, partly as the result of my thinking, and partly as the result of experiments, and I am glad to see that the progress of research has fully confirmed them. There is one point in regard to which I am not able to share the views of Dr. Dupuy; that is, his denial of the existence of vaso-motor centres in the spinal cord. Unless it is by some such central mechanism, I do not see how the peripheral portion of the vaso-motor nervous system can be brought into connection with the central nervous system. I would like to inquire as to what becomes of the fibers which pass to and from the spinal cord, connecting it with the sympathetic? It has been said that they are for the control of the action of the arteries of the cord. But it seems to be forgotten, that the blood vessels which supply the spinal cord receive their vaso-motor supply, as do other blood vessels in different parts of the body. Vaso-motor nerves follow the blood vessels themselves, and do not take an independent course, as these *rami communicantes* do, and therefore it has seemed to me that the fibers that come out from the spinal cord and pass to the sympathetic ganglia, arise out of a central vaso-motor mechanism in the spinal cord. If I have understood Dr. Dupuy correctly, he affirms that the vaso-motor system is disconnected from the spinal cord.

The spinal vaso-motor mechanism to which I have referred, may be summed up in the medulla oblongata, or in the neigh-

borhood of the so-called "convulsive centre," from which the whole vaso-motor nervous system may be excited. It seems to me that we have reason for thinking there is a connected line of vaso-motor centres the whole length of the cord, which receive impressions that pass into the spinal cord by the way of excitor nerves, the impressions being reflected again from the vaso-motor centres of the cord, outwards along vaso-motor nerves toward the periphery. Again, I am not prepared to admit that there are two classes of vaso-motor nerves, vaso-constrictor and vaso-dilator. I believe moreover that particular vascular areas of the body are related to definite regions of the spinal cord, where the corresponding spinal vaso-motor centres lie. I believe that the sympathetic in the neck, which when divided leads to changes in caliber of the extra-cranial vessels, does not contain the nerves which control the circulation in the brain, these latter nerves have their origin much higher up. They rise from within the skull itself. You may have the circulation of extra-cranial parts of the head change very greatly without the brain being affected, and *vice versa*. I do not see how Dr. Dupuy can avoid admitting vaso-motor centres in the cord and medulla.

DR. DUPUY. The fact which you have stated that the vaso-motor centres culminate in the medulla oblongata, I think can be explained as well upon the theory of classical authors as the one I have tried to establish; moreover, as vaso-motor nerves will carry impressions both ways, and as it is also impossible to make a lesion of the medulla oblongata, without at the same time making a lesion of the membranes, which contain so many of the elements, it is easy to understand how you can have the results of Bernard without having made any disorder of the centres. The mere lesion of the membranes in that neighborhood will give rise to the production of sugar in the urine. These facts which I have brought forward to-day, I have seen many times.

DR. JEWELL. I would not be understood as making any objection to facts, for when facts are brought forward, that is an end to argument. If facts lead us in a certain direction then that is the way to go: if facts show that the phenomena can be better explained without vaso-motor spinal centres,

than with them, all right. It is difficult for me to see how a connection between these two nervous systems is to be made, if Dr. Dupuy's views are correct. I do not see how we can explain a case in which an irritative impression has been directed into the cord by the way of a sensory nerve, and following this is a change in some related vascular domain, apparently only through the spinal cord. I do not see how this can be explained unless there is some central mechanism through which it is accomplished.

DR. DUPUY. There is a series of facts tending to show that lesion of the elements of which I have spoken, brings on vaso-motor paralysis, while there is not a single case on record of alteration of the cerebro-spinal nervous system, not implicating the membranes some way or other, with vaso-motor disturbances.

DR. SEGUIN. I would like to ask Dr. Dupuy how he reconciles that statement with the high temperature in cerebral hemorrhage?

DR. DUPUY. The temperature only rises after a series of reflex actions have been set up by the hemorrhage.

DR. SEGUIN. I would like an explanation as to whether pressure upon the vaso-motor centres produces the fall and afterwards the rise in temperature? At the first period we have a fall and then a rise. In the first stage it seems to me that the lesion is never complete.

DR. DUPUY. I say that the blood generally finds its way to where those ganglionic cells are located; the blood gets into the ventricles and the œdema which follows and also the congestion of the choroidal membrane, which contains a large number of the vaso-motor nerves, all go to explain the facts of elevation in temperature. In all cases of hemorrhage no matter where located, there is always within 20 minutes to 24 hours after its occurrence, a production of albumen in the urine,—polyuria, and I have frequently seen sugar also. On the other hand the fourth ventricle is so protected anatomically, that there is less pressure on it than in other organs, when there is hemorrhage in the brain.

DR. LORING. Is there any proof that with hemorrhage in the brain we have an increase of tension in the vessels of the

retina? It is the exception to find choked disk in these cases.

DR. SEGUIN. I do not think there is any means to determine that. I think you will acknowledge that choked disk does not invariably express pressure.

DR. DUPUY. I think there is an experiment which shows that tension in the arteries is augmented in hemorrhage of the brain.

DR. SEGUIN. I always believed there was pressure during hemorrhage.

DR. LORING. Some say that in apoplexy and other lesions of the brain substances, choked disk does not take place, and hence it has been argued that there was no pressure with hemorrhage.

DR. JEWELL. I do not see how the experiments referred to by Dr. Dupuy, show anything about intra-cranial pressure. I do not see how the experiment in which you take the carotid artery, which is outside of the cranial cavity, gives any just idea of intra-cranial, as distinguished from vascular pressure elsewhere. It may be shown in a vessel of the leg by this method as well, perhaps, as in the carotid artery. I do not see how increase of pressure in that vessel can teach us anything specially as to intra-cranial pressure.

DR. DUPUY. When the pressure is increased in the carotid artery, it is in the brain also.

DR. JEWELL. I do not wish to prolong the discussion, but I am not able to see, thus far, any answer to Dr. Seguin's question, in regard to the greater elevation of temperature on one side of the body as a result of cerebral hemorrhage. In at least many such cases, there is no lesion at all, as far as is known, of the dura mater itself, or the cortex of the brain. If these parts are not involved, I do not see how, or through what mechanism the change in vascularity and temperature in the opposite leg and arm, can be explained. I want to know, if possible, what the mechanism is. If it is extra-spinal, I want to know how it can be limited to one side? I want to know what vaso-motor apparatus connects the cerebro-spinal axis with the peripheral vessels of the opposite side?

DR. DUPUY. I will try and make more clear the answer which I gave to Dr. Seguin's question. When there is a very

small hemorrhage in the brain tissues far from the membranes, there is no elevation of temperature in any part of the body. In order to give rise to an elevation of temperature, it must be a hemorrhage of larger extent, and the elevation is more marked as the hemorrhage is nearer to those membranes which I have said contain those centres. When it is in the cortical region, it is still more easy to explain, considering that this part is really vaso-motor, on account of the proximity of the pia. That is the way in which it takes place in a direct manner. When it takes place in a reflex manner, it is by acting upon those cells which are in connection with the spinal cord. I must state that all I have said is a mere outline of my studies, which are in course of prosecution.

DR. SEGUIN. I fail to see, if the vaso-motor centres are broken up and distributed to the several parts of the body, how we can account for the rise in temperature which I have seen in many diseases; and how we can have vaso-motor paralysis following injury to the spinal cord. It is an eminent fact, that with hemisection of the spinal cord, we get on the same side vaso-motor paralysis, and on the opposite, anæsthesia.

DR. DUPUY. The vaso-motor paralysis in hemisection of the spinal cord, does not always involve the whole side, and also, it does not last so long as paralysis of motion. It is at present one of the most vexed questions, as the researches of Brown-Séquard, Schiff, Sanderson, Hutchinson, and others, bear witness. You must remember that as I say, the vaso-motor nerves must be injured when the spinal cord is cut across, since you cannot divide it without injury to the membranes which contain the vaso-motor nerves. You must observe that motor paralysis can be obtained without vaso-motor paralysis in the process of disease.

DR. SEGUIN. If these are distributed, as you say, why do they not continue to act after hemisection of the cord?

DR. DUPUY. Why do not the ganglia of the brain, after a mere pricking of one of them, act?

DR. JEWELL. I did not know that by simply scratching the pia mater you would have the same phenomena as when you make a hemisection of the cord.

DR. DUPUY. Yes, and moreover, you will have anæsthæsia following.

DR. JEWELL. Yes, I am prepared to believe that; but not that the same phenomena follow as in hemisection.

DR. E. C. SEGUIN read a report of seven cases in which more or less limited lesions of the cerebral cortex had been connected in life with definite symptoms.

The cases were grouped in three categories.

1st. Cases in which a limited lesion produced aphasia with or without hemiplegia. In Case I., *ramollissement* of the convolutions, in front of and just behind the fissure of Rolando on the left side, produced right hemiplegia and complete aphasia. Much recovery of motion occurred; aphasia remained complete. Careful examination of brain showed that the posterior part of the third frontal convolution and the folds of the island of Reil were involved in the lesion. In Case II., embolism of the first part of the left middle cerebral artery produced right hemiplegia with complete aphasia. There was *ramollissement* of the superficial part the left third frontal convolutions and of the first two folds of the island of Reil and their adjacent white matter. In Case III., chronic aphasia of varying degree, epileptic attacks, and partial temporary right hemiplegia were found to have been caused by a chronic pachymeningitis adherent to the altered left third frontal convolution near the fissure of Sylvius. Death in status epilepticus, with complete right hemiplegia, caused by extensive central softening of left hemisphere; very recent compared with meningitis. In Case IV., a secondary (by infection) tubercular meningitis, with its focus about the left middle cerebral artery, and over the third frontal convolution and the folds of the island of Reil, produced intermittent aphasia (occurring almost every forty-eight hours), and at the close of life, in twelve days, there were complete aphasia and right hemiplegia.

2d. Cases in which a limited lesion produced paralysis. Case V.: Small patch of softening in the middle of the left ascending frontal convolution, reaching inward to roof of ventricles, produced a right hemiplegia without aphasia, and probably without facial palsy. When examined, had palsy of right arm, none of face or tongue, and was paraplegic (from spinal lesion found in cervical region).

3d. Cases in which a limited lesion gave rise to limited convulsions. Case VI.: A young man, injured on top of head, developed common epilepsy, replaced by local epileptiform spasms in left face, neck, hand, and arm; paresis of these parts. Later, left hemiplegia and return of general spasms. Autopsy showed injured skull, thickened bone, adherent thickened bony dura over upper (inner) end of left ascending and first frontal convolutions, a sarcomatous tumor, starting from adherent dura and penetrating into nearly whole of upper half of right hemisphere. Case VII.: Septicæmia, suppurative meningitis, and two abscesses in cortex of brain. One as large as an almond in lower part of left second frontal convolution, not involving more than anterior margin of the third, gave rise to no symptoms. The second, about the size of a pea, situated in the white matter just under the cortex of the middle part of the right second parietal convolution, was in all probability the cause of very singular epileptiform (no loss of consciousness) spasm, which occurred early in the disease, in the left hand, arm, and face.

These cases seem to favor recent views on the localization of function in the cerebral cortex. The first group emphatically supports Broca's hypothesis of a speech centre in the left third frontal convolution, and the anterior folds of the island of Reil. The cases of the third category also give support to Hitzig's and Ferrier's views. The author is not prepared to accept so fine a localization as proposed by these gentlemen, but it seems to him that we are in a position to speak of excitable and non-excitable regions in the cortex (pathologically speaking.)

DR. CROSS. I think the thanks of the Association are due to Dr. Seguin for his valuable paper. I know of no way in which we can advance our science to such an extent as by an accumulation of clinical cases, accompanied with post-mortem examinations as carefully recorded and as accurately localized as have been made in this paper, together with such thorough microscopic investigations. I do not propose to discuss the subject of localization of the functions of the brain. We are not yet prepared with sufficient clinical facts to either affirm or deny that these different motor centres exist in the human subject,



as have been demonstrated by Fritsch and Hitzig to exist in the lower animals. I do not believe we can say that they are right until we have further confirmed their experiments.

DR. DUPUY. I am greatly interested in the paper of Dr. Seguin. There is something which strikes me as interesting in this paper; it is that he has a number of cases which show a relation between lesions and symptoms, and others which do not. That is to say, some which support Fritsch and Hitzig's theories, and others which do not. There is no greater error in science, since the doctrines of Longet concerning the spinal cord, than the teachings of Fritsch, Hitzig, and Ferrier. The error in both cases was of the same nature, and arrived at by the same method. Longet pretended that as the application of electricity to the posterior columns of the cord caused pain, therefore those columns were tracts for sensory impressions, but nobody believes that now. In like manner the localizers having used electricity, and seeing movements following irritation of the cortex, with that agent, concluded that the parts irritated were motor centres. Prof. Charcot has himself published a case, which is to be found in Trousseau's clinical lectures, which goes entirely against the aphasia theory. Again, Prof. Charcot has himself stated that persons suffering from lesions of the internal capsule were permanently disabled because that centre was destroyed. Now he has since published a case of lesion of the internal capsule, of 15 years standing, I believe, where not only all of the phenomena were observed, but also hemichorea; and yet all those symptoms disappeared entirely for several days after the application of metallic bodies, in the manner of Dr. Burq. Now how could that be, if a centre had been destroyed, for the internal capsule was destroyed before, during and after the application of the metals? And yet we have the changes in the phenomena observed.

DR. SEGUIN. With reference to Charcot's first case, was the question discussed in regard to the left and right handedness?

DR. DUPUY. Yes, sir, Trousseau has discussed those subjects.

DR. SEGUIN. I think there is great doubt in regard to the old cases. With reference to Dr. Spitzka's own case just announced, I think it is one of possible great value, and should

be examined with great care. I had a case last year in a boy with supposed syphilis of the brain and right hemiplegia. We found a tumor of the right hemisphere near the apex of the sphenoidal lobe. We made some cuts through the healthy hemisphere, and we were satisfied that there was no lesion in it. I put the brain in bi-chromate of potash, and it was my intention to present it to the Pathological Society and have it examined by a committee, without my being present, and thus have an unbiased opinion, but it decomposed. I should be unwilling to publish a negative case of such importance without the support of a committee. I attach an inferior value to negative evidence.

DR. JEWELL. I do not intend to prolong the discussion. Notwithstanding you have a few cases in which the lesion is on the same side as the paralysis, yet there can be no question but that in the majority of cases it is the other way. I can never give up the general idea of localization of functions in the brain, until I have better reasons for so doing than I have yet known. But as to what part of the brain exists for particular functions, I am sure we cannot with certainty say.

On motion, the Association adjourned, to resume the discussion in the evening.

The Association was called to order at 8 P. M. by the President.

Present: Drs. Jewell, Miles, Emerson, Dupuy, Spitzka, Hammond, Cross, Beard, Seguin.

On motion, the reading of the minutes of the preceding meeting was dispensed with.

On motion of DR. G. M. BEARD, it was resolved to hold the next annual session of the Association in the city of New York.

On motion of DR. T. M. B. CROSS, of New York, the Recording Secretary was directed to preserve fifty copies of the first volume of the Association's Transactions.

On motion of DR. E. C. SPITZKA, a vote of thanks was tendered to MR. JOSIAH ROBERTS, of the New York MEDICAL RECORD, for his attendance and note-taking during the entire session, thus securing a full report of the proceedings of the Association in a widely circulating medical journal.

Before the discussion of Dr. E. C. Seguin's paper was resumed, DR. EUGENE DUPUY made a few remarks upon

#### HEREDITARY TRANSMISSION OF PECULIARITIES

It was a report of a curious case of heredity. Dr. Dupuy stated that he owed to his friend, Dr. Gibney, the opportunity of observing a family consisting of father and mother, five children, and one grandchild. The father and mother are semi-ambidextrous. All of the children and the grandchild are ambidextrous to an annoying degree; all of the movements which they perform with one hand are simultaneously performed by the other hand. The girls are obliged to use only one hand when dressing themselves, or when cutting patterns, and hold the other hand down by their side, because the two hands perform the same movements at the same time, and would interfere with each other.

Attention was called to the fact that the father of the grandchild is not semi-ambidextrous. Dr. Dupuy has made experiments upon these persons, and has found that, if the skin of the forearm on one side be kept well dry, and a rapidly interrupted electrical current be used, so as only to call forth reflex actions, it is possible to induce synchronous movements in the fingers of both hands, and also muscular contraction in the lumbricales muscles of the fingers, which are too rapid to be carried on by the will. Dr. Dupuy considered these facts of great interest when coupled with the facts which he reported yesterday about hereditary epilepsy.

DR. JEWELL. In this connection, I will refer to a case belonging to a similar class to the one referred to. A medical student of mine relates the case of a boy who fell from a height upon the back. Paralysis of the legs followed, but in one or two years movement again became possible. The resumption of voluntary movement was like this, when he willed to move his right leg his left leg would move, and *vice versa*. Whether it may have been a delusion of the patient as to his willing to move one leg rather than the other, I cannot say.

DR. DUPUY. There are many such cases upon record. I know of four cases which are identical with Dr. Jewell's, and are authentic. The two last that I remember, were published by Dr. Hayem and Dr. Onimus, of Paris. In these cases, the

phenomena depended upon lesions localized in the spinal cord, whilst the cases I have reported to-day seem to have a cerebral origin, as voluntary movements as well as involuntary movements are bi-lateral.

DR. SPRITZKA called for Dr. Seguin's views in regard to the subject of his last paper—Localization of Brain Lesions—which was given, and an animated discussion followed.

DR. SEGUIN. Perhaps I should have stated my opinion upon localization in my paper, but as I did not, I am now very glad of the opportunity of placing myself on record this evening, and reproduce what I have taught here in this amphitheatre for several years. I think there can be no doubt that irritations of different parts of the brain do excite motor manifestations. The application which I make of this proposition to my practice and thought, is to use the facts as a guide in study. I do not accept the hypothesis of Ferrier, although I consider it a very important hypothesis. Before the students at my clinic, I have attempted to localize lesions in the brain, and have felt no hesitancy in localizing the lesion near or in the island of Reil in cases of clear aphasia. With reference to my communication this afternoon, I am free to say that some of the cases there given show that certain parts of the brain are very excitable, and that certain other parts are not very excitable. If I were asked to give my opinion in regard to localization of excitable and unexcitable regions, I would say that the anterior part of the frontal lobes and the posterior lobes are not excitable, but that the median parts of the cortex are.

DR. SPRITZKA. It is one question whether there is such a thing as localization, and quite another thing whether the experiments of Hitzig are of any value. In regard to the second statement, I do not think that Dr. Seguin was right in saying, that all writers agree as to the experiments of Ferrier. Fuerstner, for instance, differs in his results. In regard to the paper this afternoon, I had an autopsy this day of a patient in whom the convulsive movements were very marked in the right facial muscles and arm, but there were none in the leg. This would have been a case for special localization. I found meningitis of the right cerebral hemisphere and the superior sur-

face of the cerebellum, which extended symmetrically for a distance of three centimetres. The lesion was one purely of the right side, as regards the hemispheres, and the lesion was on the same side as the convulsive movements.

DR. DUPUY. The case of Dr. Spitzka is right against the localization theory: the lesions were on the right side in the cortex, and in the so-called motor regions; and the symptoms on the right side in the limbs. If there were unseen lesions in the left hemisphere to explain the symptoms on the right side of the body, why were there no symptoms on the left side of the body to explain the lesions in the right hemisphere, since the cortical lesions on the right side occupy those areas of the gray matter which are called psycho-motor? If there are lesions in the left hemisphere to which the symptoms on the right side are to be referred, then it must be admitted that the cortical region on the right side does not contain centers, as there are no symptoms in the left side to be ascribed to them; which is contrary to the doctrine of localization. I do not agree with Dr. Seguin when he says that all physiologists have accepted that there are excitable and non-excitable regions of the cortex of the brain. I have shown long ago that it is not so, and so have Goltz, Schiff, Brown-Séquard, and many others. Moreover, Ferrier himself has shown that irritation of the *gyrus angularis*, which is, according to him, a sensory center, gives rise to motion, but he pretends it is true that that motion is the awakening of organized passed experience; the motor manifestation of a sensory impression, of course this is untenable. Schiff has found what I have myself discovered, that during complete anaesthesia the irritation of the cortex gives rise no more to contraction of muscles which already reflex action cannot be obtained by excitation of the sciatic nerve, although direct action on the gastrocnemii muscles is obtained; therefore, when the irritation of the cortex is not responded to by any movements, it is not because it is anaesthetized, for if the irritation be applied to the tissues underlying the cortex, which is removed, we have movements at once. It should be remembered that the cortex of the brain is said to be excitable, and yet unlike other excitable nervous matter, it can only be excited with electricity; I have shown

abundantly that the action of electricity cannot be localized to any particular spot. If, as Ferrier puts it so forcibly, the irritation by electricity of one point of nervous matter will always give rise to one and the same kind of phenomena, whilst irritation of another point very near will not have a similar effect, I do not see that it proves anything further than this, that electricity being a physical agent, acts in accordance to certain laws in a constant manner every time that identical circumstances are present. Now the convolutions are separated by sulci, which are channels of conduction to those parts of the brain which are known to be excitable, according, therefore, as electricity is carried or not to them, we shall have or shall not have movements.

But Ferrier says that there is a case taken from human pathology which confirms the doctrine; it is that experiment of our associate Dr. Roberts Bartholow. Now, any one who will take the care to read the account of this experiment, will see for himself that it does not prove anything since the author inserted the needles, insulated up to their point, two inches deep into both hemispheres in their posterior region, so that the cortex which was destroyed in part by the cancerous growth, of which the patient was suffering, could have in no wise been irritated in that experiment. All the experiments that I have made up to this time show that the gray matter in animals is not excitable.

DR. JEWELL. I cannot say that I have performed experiments, but notwithstanding this, I feel that I am in a position to interpret the work of others. I have not supposed, for a long time past, that the gray matter of the cortex, or of the spinal cord, is excitable by any known artificial means. The gray matter, the cells, if ever acted upon, as they must be, are never excitable except by the channel of their fibres, therefore the conclusions drawn from one whole class of experiments which have been made, must be rejected. The fibres which lead downward from the gray matter are the excitable parts, and not the gray matter itself.

The cortex forms no part of the motor apparatus proper. It is only an excitation mechanism, from which impressions pass downward to the true motor apparatus which

culminates in the basal system. I must yet hold that different parts of the cortex of the brain have different functions, at least in the present unsettled state of experimental physiology.

DR. DURY. All I wish to say is, not that the cortex has no functions, but that what the localizers state is unsound. I believe we ought to stand by Flouren's doctrine.

DR. SPITZKA. I have endeavored to show that no number of experiments will satisfy us that lesions on one side will produce effects on the other side of the body. If there is anything that is exact, it is anatomical research, and this shows that there are undoubtedly variations which may explain why we have sometimes lesions on the opposite side, and sometimes on the same side with the convulsions or paralysis, and these variations are to be sought for in the greater or lesser completeness of the pyramidal decussation. But I cannot admit that each of the complex functions of the mind, or even all of the simpler ones, are situated in one, and only one cortical area, as the former are but the compounds of simpler functions, their anatomical seat must correspond to these simpler functions taken in the aggregate, or rather, it has its seat in several perhaps distant cortical territories, as well as in the associatory tract of fibres which connects these. In regard to Dr. Jewell's statement, I wish to be distinctly understood as saying, that each group of nerve cells in the cortex must have its function.

DR. HAMILTON. It has struck me that more attention should be paid to the study of localized meningitis. In meningo-encephalitis, the motorial results of cortical irritation are quite distinctive. That the physiological experiments of Hitzig and Ferrier have had ample confirmation by cases collected by Charcot and others, there can be no doubt, and I have recently seen a case of epilepsy in which local spasms of one hand preceded the general convulsions. After death, an exostosis was found pressing upon the centre, which is supposed to preside over the movements of the hand and arm. Landouzy has collected over one hundred suggestive cases, to which we must admit, those of Dr. Seguin form a valuable addition.

DR. DURY. I have spoken so much lately about the part played by the membrane of the brain in the production of

movements, that I will say no more. Now, as for the spicules of bone pressing upon the motor centres and producing paralysis, there are a great number of such cases. That fact does not show much. Dr. Proust relates the case of a man who received a blow with a bayonet, and there were spicules of bone which, according to measurement, were pressing in the region of the angular gyrus. The patient had paralysis of the face and arm on the same side, and aphasia. No sooner were the spicules of bone removed, than at once he recovered; after the dressing of the wound had been completed, it was found that the dura mater had not even been torn by the bone. It is useless to state that the lesion was far from the speech centre. The ideas of Landouzy are the same as those of Rendu, Wilks, and before those, Bouchut, and Cazauvielh, and many others who have written a great deal on the subject of arachnitis.

#### MENTAL THERAPEUTICS.

This was the title of a paper read by DR. GEO. M. BEARD, and was a continuation of a paper which he read last year before the Association, on "The Influence of Mind in the Causation and Cure of Disease."

Dr. Beard cited two cases of organic disease of the spine, which had received decided temporary relief while sitting under blue glass.

The opinion was expressed, that in time all works on therapeutics must include a chapter on mental therapeutics.

From experiments and study, Dr. Beard was able to make the following psychological suggestions:

1. The ill-success of patients treating themselves, and of physicians treating their own families, was partly due to the want of awe and the emotion of wonder to co-operate with them.

2. The old custom of keeping patients ignorant of the contents of prescriptions by writing them in Latin, had psychology on its side.

3. It is entirely possible that hydrophobia and tetanus may be brought on by mental causes, with all their distinctive symptoms, and that death may result through the emotions of fear and expectation alone.



4. Patients whose will and intellect are feeble have a bad prognosis, for with them the objective symptoms are trifling; and *vice versa*.

5. Physicians of great scientific attainment and real worth, may fail when an ignorant and obscure charlatan succeeds, because in the latter wonder and awe are excited, and these are more powerful therapeutically than simple respect.

6. In experimenting in hospitals with new medicines, patients must be deceived, or else the results are complicated by mental influence.

Dr. Beard expressed a belief that those who would repeat his experiments would confirm his results and conclusions.

DR. SEGUIN. I did not have an opportunity of expressing my views upon the subject of Dr. Beard's paper last year. had I been present, I would have sided with him in saying that the emotions of patients may be made the means of curing them of a disease. I deny, with Dr. Beard, that there is any trickery in the use of the emotions to this end. We do many things in the practice of our profession (such as exposure of the person, mutilations, etc.,) which, considered *in themselves*, apart from the *end in view*, might be termed indecent and barbarous; yet we think it is right to use these means. Again, in our prescriptions we give certain assurances, the fulfillment of which we are not certain of. In prescribing hypnotics, a very uncertain class of remedies, we tell the patient, "That will make you sleep," and the hypnotic effect is increased; the drug has a better chance to act. In many cases, the physician's conscience must be the higher law.

This paper was further discussed by Drs. Dupuy, Miles, Seguin, Beard, Jewell, Hammond, and Cross, pending which, the society adjourned.

E. C. SEGUIN, M. D., *Recording Secretary*.

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